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ABSTRACT

The purpose of this paper is to make some comments designed to stimulate some unconventional thought in connection with the problem of developing sociological theory. The author questions the extent to which social science research follows scientific procedures based on a "hypothetico-deductive" format and the extent to which its propositional structure is derived from theory. Arguing against the informality of social science procedures, the author seeks more textual closure by defining terms, unpacking the verbal formulations into simpler components, analyzing the nature of statements, identifying all variables, and making explicit the relationship between them. The report includes an examination of some underlying assumptions about social phenomena derived from linguistics research. Rather than supporting deductive theory, the author suggests that the interpretive form of understanding expressed in ordinary prose can be used to discover facts amidst the complexity in which they exist. (Author/LAA)

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INADEQUACIES IN FORMAL APPROACHES TO THEORY DEVELOPMENT IN FAMILY SOCIOLOGY AND SOME IDEAS ABOUT ALTERNATIVE PROCEDURES

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(The single-spaced format of this paper has been adopted not only to conserve natural resources but also to cut down on expenses of duplication Apologies to any who are discomforted by the lack of space between lines.)

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I. INTRODUCTION: RECENT HISTORY OF FAMILY SOCIOLOGY'S ATTENTION TO THEORY DEVELOPMENT

It is probably fair to say that concern for theory is as great in the field of sociology of the family as in any of the fields of sociology. This concern is explicit in intellectual interest and in organizational form as many sociologists struggle with the problem and as groups organize, as Reuben Hill has organized such a group at The University of Minnesota, to struggle with it. The purpose of the present paper is to make some irascible and contentious comments designed to stimulate some unconventional thought in connection with the problem of developing theory.

The present concern for theory extends back to the mid 1950's where Hill, Katz, and Simpson (1957) made an initial attempt to delineate conceptual approaches to family study. The attempt, in hindsight, seems best understandable as an attempt to delineate and codify the intellectual and academic biases that seemed to guide the formulation and execution of family research. It should be understood that the term "biases" is not being employed pejoratively. Finer, more explicit, and more extended attempts followed (Hill and Hansen, 1960; Christensen, 1964: Chapters 2-5; Nye and Berardo, 1966). In addition at least one recent review has been written to check on how well the field has conformed to these codifications (i.e., how well it set about to constrain writing to one or another of the codified approaches) since the attention to the approaches became explicit (Klein, Calvert, Garland, and Paloma 1969).

Especially in the later attempts, the emphasis on codification began to give way to a concern for methods of theory development. This became directly explicit in Hill's writing in 1966 (Hill, 1966). One of the trends in the emphasis on theory was the conviction that in the interests of developing theory formal strategies such as that proposed by Zetterberg (1963) would be appropriate. In taking this tack family sociologists were, it seems, constituting a special case of a general trend in sociology and the social and behavioral sciences in general toward confidence in formally stated deductively integrated propositional systems. A particularly concise statement of this position is provided by Homans (1964) and a quite convenient summary of this position in terms of the general relationships between logic, knowledge, and social science was provided by Brodbeck (1963).

Almost as if to follow the old bureaucrat's bromide that things achieve structural perfection only at the point of functional collapse, these unusually lucid statements seemed to mark the highwater point of confidence in the logical-positivist format. Immediately after the mid 1960's things began to deteriorate.

A number of strategies that seemed to have great promise in the early sixties turned out to be other than what was anticipated by the early seventies. Speaking more particularly of the field of sociology of the family, the idea of taking inventory of propositions seemed to be of great interest. A number of such inventories of varying scope have in fact appeared (Goode, Hopkins, and McClure, 1971 is exemplary). But instead of giving the impression that progress has been marked by their publication, these inventories leave the reader with a sense of futility. Seemingly an inventory leaves the theory developer with the raw materials for a logical-positivist theory building enterprise: all that remains are problems of assembly. But what is one to do in the face of dozens, hundreds, and even thousands of propositions themselves composed of terms of uncertain definition and level of generality or abstraction. The very production of such inventories suggests the exhaustion of any obvious hopefulness in connection with the strategy of building broad comprehensive propositional systems. These inventories, instead, are of enormous utility as a more discerning format for bibliographic enterprises.

A second strategy of great promise was the sophisticated statistical techniques that came to great profusion in the sixties, especially path analysis. Although those responsible for the introduction of the path analytic methods into the general stream of sociology seemed to deny any special status for them, some special status seemed to have been assumed in the "aura" that surrounded them. The pictorial flow charting representation was particularly compelling to many as was its systematic statistical handling of linkages between changes in the values of variables. These linkages, when referred to as "causal", resulted in a sort of swooning, a collective blowing of cool. These methods lend a greater degree of descriptiveness to empirical inquiries because they furnish a more satisfying representation of "what is going on" than other methods yet advanced in statistical analysis but they probably are not serviceable in the interests of broad comprehensive theory. They are best thought of as particularly useful to organize a given empirical study around or to keep account of findings from a line of similar studies of the same (or roughly the same) set of variables. But they are not a way toward a broad organizing framework for the statement of knowledge in a whole substantive area.

Either because of this or concurrently with it the position in connection with theory development has become quite a bit more complex in the field of family sociology. Having become explicit, the conceptual frameworks codification idea never gained a commanding hold on the organization of inquiry. By 1969 only 15.2 per cent of marriage and family articles surveyed in 12 major behavioral science journals for the years 1962 through 1968 were ones in which there

was an attempt to utilize a specific conceptual framework (Klein, Calvert, Garland, and Paloma, 1969:686). Shortly thereafter Aldous (1970) sought to summarize attempts to develop theory in sociology of the family not in terms of distinctions between categories of concepts employed but in terms of more complex distinctions between "strategies" that seem to be based on the answer to the question: "What kind of idea about theory is being used?" In effect, this sort of approach is much more of a description of the actual natural intellectual operations involved in the field at this point in time. It presumes that there is less consensus on concepts than the conceptual frameworks approach does and it specifies that there are other differences to be addressed than differences in conceptual status. Still more recently Hill (1971) has restated his position in a presentation quite critical of conventional methods and has himself adopted the "strategies" as opposed to the "conceptual framework" approach.

II. THE PRESUMED GOAL OF THEORY DEVELOPMENT ENTERPRISES.

What has been said so far can be read to imply either of two things. The first implication is that we have a lot of work ahead of us. If one pales at the massiveness of a propositional inventory, this speaks to his lack of heart. He should write for a big grant and get to work analysing, sorting, etc. If one is frustrated by the operational limitations on path analytic methods maybe something is around the corner that will transcend these limits. The heart of this implication is that there is nothing basically wrong with the plan, there is just too much information to make sense out of at the present time. A second implication is that there is indeed something wrong with the basic plan.

This paper calls attention to this second implication. In what follows a case will be made that we may have to renovate our ideas of what it is that we are pursuing. Whatever it is that a particular social scientist favors as his strategy of achieving theory, all social scientists of the present day would concede that it be presentable in some linguistic format. That is to say it must be capable of being stated in either spoken or written form with the latter much to be preferred. This point is so obvious that the reader is likely to be confused by the simple fact that it is stated. What else could a theory be but either written or maybe spoken? What else is any report or essay in science? Obvious or not, the language nature of theory should not blind us to the fact that whatever problems there are in the relationship between language and nature, these same problems prevail in the relationship between theory and phenomena. The difficulties that language has in representing nature are the same ones that theory has in representing phenomena.

The impact of this point is not likely to be great at first blush even when grasped because it will be assumed at first that though these problems

are great, they have in fact been addressed. Even if not solved they are admitted as problems to be worked on in the ordinary course of doing science; they are being attended to. The mechanism of attention here is logic and deduction. Symbolic logic, in this view, is a systematic language that eliminates the equivocation, hidden assumption, internal contradiction, etc., etc., that plagues ordinary language. Reconstructed languages are the superior mechanisms of statement in science and enable us to dispense with the confusing and obscuring languages-in-use. In this vein ever more explicit propositional systems stated in reconstructed formats and manipulated by highly refined mathematical procedures is the goal. It is fair to say that most social scientists, though differing in preference for strategy as to how to get there, would affirm that this is where we want to go and that such a propositional system would adequately represent (or predict, if you so desire) nature, i.e., social phenomena.

It will be pointed out that: (1) we are in fact further from this ideal than most social scientists would probably assume, (2) the ideal is probably unattainable in principle and (3) there are alternative ideals.

III. ACTUAL PROCEDURE OF SOCIAL SCIENCE ACTIVITY.

In general most social scientists in their research endeavors follow a format that they would assume. constitute a relaxed version of the strict scientific format. Thus they frequently employ a body of more or less primitive theory. That is they use some sort of stock of ideas taken from classical or recent written literature and imply a set of testable hypotheses from this stock. They then go out and gather data and make decisions on the basis of statistical principles (or simpler ones) about whether the data supports or casts doubt on the hypotheses. They then reason back up to the stock of ideas and make comments on these ideas that are based upon the fate of the hypotheses.

Now all of this is a caricature of the strict scientific (or "hypothetico-deductive") method. The current feeling in the social sciences is that his is a reasonable enactment of that method given that things are the way they are. That is to say: given that we can't define most of our terms, and given that we do not suspect a good many of the relationships that may actually be the case, and given that we may be asking the wrong questions in the first place, and given that we can't at this time specify all the variables, and... etc., etc. The point is that there is a feeling that we are doing what we can in pursuit of the classically conceived scientific rigor with the tools at hand.

But this falls very far short of the classical degree of rigor. So much so that it is probably in poor taste to really insist that we are following the basic hypothetico-deductive format. Our propositional structure (i.e, our sets of hypotheses) are not rigorously derived from theory. Ultimately their relationship to theory is not a formal deductive relationship but rather

a looser and more open one in which,if nobody point out any problems with it, all who read it will acquiesce in it. The body of theory itself is of "open texture." That is, it is not a tight system of deductively interrelated statements explicitly formulated and logically sound. It is not even an approximation of this. It is just as termed above, a stock of ideas. It is persuasive in a discursive sense but its formal structure remains unexamined. In general the deductive relatedness of the formulation is not even addressed. We do not even conceive of our theories in terms of their logical nature. If nobody exposes any major contradictions, we just go ahead with our projects. Indeed this entire paragraph is likely to be seen as a labored lingering on formality which just is not seen as the point of theory. But that is just the point!

When it gets down to brass tacks, what we do is try to say something interesting and then go out and contact empirical "reality". We poke around outside of the ivory tower trying to see if there is support or doubt out there for our ideas. Now this process of trying to find support or doubt out there is subject to some "rules of evidence." Thus the development of social scientific methodology over the years is of indispensible value. These methods constitute the discriminating processes of contact with the outside-of-the-tower world. But this is not the core of social science or of science in general. Other types of activities are as dependent upon such rules of evidence, i.e., on discriminating methodologies, as scientists are. For instance: law and jurisprudence, business or government decision making, journalism, medicine, and others. None of these fields would be devastated by the loss or lack of these rules of evidence and these methodologies to any lesser extent than would science. The point is that the research activities common <u>in</u> scientific enterprises do not define science but that what is central is the weaving together of these findings into a broader fabric of general explanation.

In any study we try and reason very rigorously and logically from our data to conclude about the fate of our hypotheses. This may be a reasonably restricted enterprise within which strictly applied logical reasoning can be effective because there are few enough variables and clearly notable relatedness between them. The point here is that logic and formality may have great pay-offs within given studies (i.e., as "rules of evidence"). But a simple extension of this strategy to the weaving-together-of-findings problem probably will not have.

Before we continue, it might be profitable to summarize what this paper sees as the current actual pattern of research activity in the social sciences: It is a pattern of:

(1) causal abstraction of propositions (or, hypotheses) . . .
(2) . . .from a body of theory (i.e., a stock of ideas) . . .
(3) . . . that is of open-texture language . . .
(4) . . . the formal deductive nature of which has not been examined . . .
(5) . . . and which is accepted conditionally . . .
(6) . . . and consensually; . . .

(7) . . . where isomorphism with aspects of the empirical world is investigated to find support or doubt . . .
(8) . . . by rules of evidence and discriminating methods.

To reiterate, this is not the hypothetico-deductive method. It is not really even an approximation of it. To quote the small cigar commerical, "Its a whole 'nother smoke."

IV. TOWARD CLOSED TEXTURE LANGUAGE? A LAYMAN'S TOUR OF RENOVATED THINKING ABOUT SCIENCE.

The last point notwithstanding, many, if not most, social scientists, it seems, would argue that the "casual abstraction" model just drawn up is just a relaxed form of the rigorous classical format. They would take the position that as a hypothetico-deductive format, if it is weak and sloppy it is only tentatively so. It can be strengthened. This may be so, but there is some compelling evidence against this position.

The obvious strategy in the face of an open-texture formulation is to close the texture. Define things, "unpack" the verbal formulations into "simpler" components, analyse the nature of statements, identify all variables, make explicit the relationships between them, and so on. It is obvious that this is an enormous task. Even the thoroughgoing attempts to formalize social science formulations, though moving in this direction, have not anywhere near accomplished this (see, for instance, Berger, Zelditch, and Anderson, 1966). These attempts are probably as far as this textural closing process will get. Moreover it is likely that they are not movements toward true formal deductively related formats but are merely highly elaborated forms of "casual abstraction." As such they are quite valuable but no more than this. The attempt to close the texture of scientific formulations is called "axiomitization" and in fact very few axiomitized formulations are known even in physics. It seems that as social scientists we anticipate formulations that are held to more rigid standards than are currently accepted by the "precise" sciences while at the same time being very far from achieving them.

It is getting very tedious to refer to some of the renovations in scientific thinking occasioned by recent mathematical developments by such figures as Heisenberg, Godel, and Turing. However, it seems that though accepted by most social scientists as fascinating, these renovations are regarded by them as inapplicable to social science at the present time. They are assumed to be so abstracted in nature that we can afford to be formalizing and closing texture for quite a while before we will run into trouble, so-to-speak. Again, this may be so, but there is some evidence that we are already running into trouble. A layman's tour of renovated thinking may be worth the effort.

In one of a number of recent summaries of these renovations, Bronowski (1966), a mathematician, discusses them as a family of limitations on logic. He points out that, " . . . every axiomatic system of any mathematical richness is subject to severe limitations, whose incidence cannot be foreseen and yet which cannot be circumvented. In the first place, not all sensible assertions in the language of the system can be deduced (or disproved) from the axioms: no set of axioms can be complete. And in the second place, an axiomatic system can never be guaranteed to be consistent: any day some flagrant and irreconcilable contradiction may turn up in it. An axiomatic system cannot be made to generate a description of the world which matches it point for point . . . "(4)The implications of this point for science is clear: "the laws of nature cannot be formulated as an axiomatic, deductive, formal and unambiguous system which is also complete. And if at any stage in scientific discovery the laws of nature did seem to make a complete system, then we should have to conclude that we had not got them right."(5)

Bronowski makes clear that the problem is with science not in any necessary arbitrariness in nature: "of course we suppose nevertheless that nature does obey a set of laws of her own which are precise, complete, and consistent. But if this is so, then their inner formulations must be of some kind quite different from any that we know; and at present, we have no idea how to conceive it."(5) The problem, as Bronowski sees it, is precisely with the reconstructed nature of scientific language: "any description <u>in our present formalisms</u> must be incomplete, not because of the obduracy of nature, but because of the limitation of language as we use it. And this limitation lies not in the human fallibility of language, but on the contrary in <u>its logical insufficiency</u>. This is a cardinal point: <u>it is the language that we use in describing nature that imposes (by its arrangement of definitions and axioms)</u> both the form and the limitations of the laws that we find."(5, underlining added.)

It is less clear whether or not this statement of the futility of formalization and logic implies a blanket limitation on the ability of the mind to extricate the laws of nature. For instance Bronowski speculates on the possibility of an informal language for physics which would be complete and consistent. That is, to say the possibility of a complete and consistent theory in natural language as opposed to reconstructed logical language. Bronowski himself doubts such a possibility but it is certainly not excluded by the established family of limitations on logic.

V. SOME ATTEMPTS TO RETAIN THE FORMAL STRATEGY IN AN ACCOMODATION WITH THE RENOVATED IDEA OF SCIENCE.

Now, so far there is no implication of instant futility for formalization. In fact so far as we have gone this issue has been part of an ongoing discussion in the <u>American Sociologist</u> for the past several years (Ferdinand, 1969; Bradley and Reynolds, 1970; Ferdinand, 1971; Gray, 1972; Ferdinand, 1973). Gra[illegible]'s contribution to this discussion notes that such

issues do not foreclose the possibility of a "quasi-general" theory of behavior (Gray, 1972). He points out that a theory of a formal axiomatic nature might be empirically complete without being logically complete (6): ". . . it is possible that the sum of facts in the empirical domain will be less than the sum of statements that the axiomatic system is able to generate and that the sum of statements in turn will not exhaust the logical truths of the system's language." Although Gray does not draw an explicit conclusion from this, he seems to imply that such a theory would be satisfactory because the problems would not be among the statements of facts in the empirical domain but would be among the residual statements, i.e., among those statements (excess to the empirical ones) necessary to bring the theory up to logical completeness. Indeed this is an interesting possibility but assumes that the problems are not well-diffused throughout the axiomatic system. Further, it counsels us to go through all of the work of developing an axiomatic theory and then take the additional steps of deducing the remainder of all possible statements to be sure that the problems are among the remainder. Obviously this last step is impossible leaving us with the doubts about the adequacy of the theory we began with. Most importantly, though, the converse of Gray's depiction is more likely to be the case, i.e., that the domain of empirical facts is larger than the set of statements in any axiomatic theory that the mind of man can deal with (in the foreseeable future), whether or not that theory is complete and consistent.

VI. A DECISIVE FORMULATION OF THE ISSUE.

Thus it must be conceded that though unlikely, there is a vague possibility that we may develop a good formalized theory as an unproblematic substructure of a larger axiomatic system where all of the ugliness is outside of the fortunate substructure. This paper, however, will argue even against this vague possibility. A particularly interesting discussion of the limits of logic is carried on by Crosson and Sayre (1967) in their exploration of the implications of cybernetics. In their consideration of the question of whether there is any fundamental difference between men and machines (18-29) they point out that one argument frequently ventured by positions that assert that there is such a fundamental difference is based upon the invalidity of an analogy between the two.

A machine is considered to be a device representable as a formal system. It is, in this argument, characteristic of machines that their operation is totally specifiable, i.e., that there is a formally organized theory of their operation. "The initial status of the machine and of its environment correspond to the axioms of the formal system, its invariant operations to the definitions, its operational procedures to rules of inference, and the results of its operation upon its input to the theorems which follow in the formal system" (Crosson and Sayre, 1967:22). Now a position such as that taken by Bronowski would assert that there are things men can do that machines, <u>in principle</u>, cannot. This is to say that no matter how complex a machine is every action it takes has a formula derived from the axioms of its system. For men however, there are actions that are taken

that have no formula that can be derived from the axioms of their systems. The formula that does govern these particularly man-like actions can be derived from informal reasoning however. In terms of this "theory of man" as opposed to the "theory of machine" there are statements"which people reasoning about the system can show to be true but which cannot be proved within the system itself " (22).

Is it in fact the case that there are instances of informally derived but not formally derivable formulae? Crosson and Sayre consider the conditions which would be necessary for the demonstration that such formulae do not really exist. They do not exist if for any aspect of human behavior it is "well enough understood to permit precise and detailed statements of its input and output characteristics" (24). That we do not have such understanding at the present time is evident, but the issue is: does the fact that we don't have it imply that we just haven't learned it yet or that it is not possible to gain such understanding?

Before this question can be addressed it is important to realize that as Crosson and Sayre have presented the issue it becomes clear that the family of limitations on logic may well bear on every aspect of human behavior or on every non-trivial one, or on a great many of them. As the issue on limitations of logic has been generally discussed it seems only applicable at extremely elevated levels of abstraction where many, many statements that involve many, many behavioral actions of many, many people in many, many settings are being integrated. If this were to be the case then it would seem profitable to pursue limited formal theories of "smaller" things because the limitations of logic would not impose themselves on small theories. We would develop the small theories and then worry about combining them into larger theories. If we couldn't combine them then we would at least have good small theories about smaller things. But as Crosson and Sayre present the issue it becomes relevant even to the consideration of individuals and small behavior systems. It is clear that a large proportion of the general axiomatic structure of an overall theory of social science may be necessary for explaining all or many aspects of human behavior. The limitations of logic, therefore, may pertain to formal theories of even simple ordinary concrere phenomena and may be said to pervade all or most formalization in the social sciences.

VII. SOME THOUGHT AND SOME EVIDENCE SUPPORTING THE PREVALENCE OF INFORMAL FORMULAE.

There is at least one group of social scientists who seem to have produced some evidence that this special "theory of man" type situation (i.e., informally derived but formally underivable formulae) in fact exists. These social scientists are the "ethnomethodologists" and their evidence deserves to be reviewed.

In a recent anthology on sociolinguistics (Gumperz and Hymes, 1972) a leading ethnomethodologist, Harold Garfinkel, summarizes two of his case

studies (Garfinkel, 1972). One of these studies was in actuality a demonstration. Garfinkel assigned to some of his students the task of explicating a short segment of a conversation. The students found much difficulty with the task. Garfinkel held them to increasingly strict standards of accuracy, clarity and distinctness. Finally, he required that they assume that a reader would know what the conversants had actually talked about only from reading literally what the students wrote. The students wrote increasingly expanded descriptions and, upon analysis concluded that each one was insufficient. Finally they gave up concluding that no description could be total and sufficient.

In their comments on this demonstration, the editors Gumperz and Hymes note that " . . . the students found the task impossible because, as would most social scientists, they took the task to be one of remedying the sketchiness of the conversation by elaborating its contents, by appealing outside the speech event to what became, under prodding, an infinite regress of context. Their error was to assume a theory of signs in which the way something is said is divorced from what is being said (form vs. content), and in concentrating on the "what", neglecting the how. In fact, the conversation was intelligible to its participants not because of some shared infinity of substantive knowledge as to what was being talked about but in the first instance because they agreed at the time on how the talking was to be interpreted. The fact that such momentary agreements can be reached, however, does not mean that content can be reconstructed later under different conditions" (Gumperz and Hymes, 1972:303).

For sociolinguistics as well as for ethnomethodology the speech event is not primarily a representation by its participants of their experiences with external reality and is therefore not dependent upon external reality for its impact. Hence "appealing outside the speech event" is futile in this view. Categories of speech events defined in terms of reference to external factors are useless if this is the case because defined in this way they have no systematic relationship with each other. Instead categories of speech event are to be analysed in terms of "how the talking is being done." These are qualitative depictions of ways of speaking. They are matters" . . . of furnishing a method for saying whatever is to be said, like talking synonymously, talking ironically, talking metaphorically, talking cryptically, talking narratively, talking in a questioning or answering way, lying, glossing, double-talking, and the rest" (Garfinkel, 1972:319).

Interaction is then a matter of sequences of such "ways of talking". The factors by which an actor decides how he will furnish such a"way of talking" as his next move after his partner has furnished a previous "way of talking" is demonstrably (and for Garfinkel already demonstrated in his case study) an interpretive matter rather than a deterministic one. "Common understanding is never simply recognition of shared contents or rules, but it is always open-ended, brought about in any given case because participants bring it about as their artful (if unconscious) accomplishment. Ad hocing remains the ultimate concern. People understand each other because 'for the while' they assume the reasonableness of each other's statements and imput and construct reasonableness, where needed, out of often fragmentary data" (Gumperz and Hymes, 1972:304).

The very notion that sets of rules formally derived and logically consistent, etc. is responsible for the orderliness, rationality and accountability of everyday affairs is in this view absurd. Any set of rules is essentially incomplete no matter how elaborately specified. The integration of everyday affairs is a "contingent, ongoing accomplishment," and is a process of interpretation. This orderliness, rationality, and accountability is then to be sought in a "theory of man" type formula, i.e., one that is informally (interpretively) derived but formally underivable.

VIII. THE INFORMALITY OF SOCIAL SCIENCE PROCEDURES: "AD HOCING".

The implications of this for social science is made somewhat more clear in the second of Garfinkel's case studies. This study has essentially an investigation of the factors involved in acquiring intercoder reliability in a study of procedures in an outpatient psychiatric clinic. Two graduate students examined clinic folders and coded the contents into categories for subsequent analysis. Intercoder reliability, if high, furnishes credence to the coded events as actual events of clinic activity. Garfinkel found that in their work the coders were making assumptions about the clinic activities that the coding methods were intended to produce descriptions of. Garfinkel then decided to examine the coding process itself. Just what were the ways in which the coders made decisions.

It was found that coders could not come to decisions without "ad hoc" considerations. These considerations are not provided for in the specifications for the use of coding categories or, in fact, anywhere else in the "official" set-up of the research project but were necessary for the coders' ability to assign documents to categories. Attempts to eliminate this "ad hocing" made coding impossible. Now normally the design of coding schemes seeks to eliminate "ad hocing" as a flaw in procedure. "The prevailing view holds that good work requires researchers, by extending the number and explicitness of their coding rules, to minimize the occasions in which . . . [ad hoc procedures] would be used." (Garfinkel, 1972:313). But it is Garfinkel's point that to do so would entirely undermine the coder's sense of the relevance of the coding instructions to the given situation he is analysing." To treat instructions as though ad hoc features in their use was a nuisance, or to treat their presence as grounds for complaint about the incompleteness of instructions, is very much like complaining that if the walls of a building were only gotten out of the way, one could see better what was keeping the roof up" (Garfinkel, 1972:313).

The implication here is that coding is the same kind of open-ended interpretive process as that involved in explaining conversations. Logical operations on coded data assumes that the formal coding instructions are the only "instrumentation premises" in the chain of logic but in fact they are not. The "ad hocing" procedures must (but cannot) be specified and brought into the logical chain if there is really going to be a fully integrated deductive system. The situation is not otherwise in other methodologies. For instance respondent "ad hocing" is brought into the situation

with survey methods. To sum up, data reaches social scientists in a linguistic process that is of the same nature as that which occurs between ordinary people. We are engaged essentially in a mediated sequence of "ways of talking" with people we study. For a discussion of this issue with direct relevance for research on the family see Cicourel (1967).

Garfinkel implies that there is no alternative to the "casual abstraction" process for social science. An attempt to close the open texture of social science language would be futile. Even a theory that at present is quite modest would expand beyond management very quickly and would move toward infinite regress. It would very soon loose deductive strength as more and more non-logical arguments assumed a larger share of the explanatory load inasmuch as "interpretive" factors were being identified as the actual operating account for the phenomena under examination. Gumperz and Hymes (1972:306-308) themselves speculate on the nature of a science under these (assumed) realities: "one gains the impression that all the investigator can do is to collect and exhibit instances" (306). Such a science is more familiar to linguistics perhaps than sociology in that for the former descriptive linguistics is a recognized division of the field though its lack of theory is also recognized.

IX SOME UNDERLYING ASSUMPTIONS ABOUT SOCIAL PHENOMENA DERIVED FROM LINGUISTICS RESEARCH.

The underlying argument in the critical position of Garfinkel seems to be a radical dependence on a theory of language that is radically different from those that proceeded it, Chomsky's Generative-Transformational Grammar (Chomsky, 1965, 1969). To completely understate this theory so as to enable us to move on, it is a theory which asserts that language is governed by a quite remarkable system of rules that enables speakers to generate and understand meaningful utterances that they have not previously heard. These rules enable speakers to differentiate between ambiguous meanings of a single utterance (e.g., "the shooting of the hunters") that cannot be differentiated on formal grounds and to supply meaning where none can be supplied on formal grounds (e.g., the reading of some poems in Carroll's Alice in Wonderland). These rules are not learned in the traditional sense but are species-specific structures. A child, after being exposed unsystematically to an irregular and inexplicit sampling of these rules in the ordinary casual speech of those around him, comes by about 4 or 5 years of age to be able to use the entire body of rules naturally and comfortably. The position of ethnomethodological theory with respect to this approach to language is made explicit by Cicourel (1970a, 1970b).

By any means of reckoning Chomsky has pointed out that an extraordinarily complex set of phenomena is implied by very ordinary and routinely observable features of language. Whether this complexity is in fact of the nature of the interpretive processes discussed above, and therefore fundamentally infinite and unanalysable, is by no means settled. Certainly ethnomethodologists seem to enjoy trading on the mysterious status of the underlying remarkable

system of rules. The enormous difficulty in accounting for the understanding and production of utterances never previously heard by the speaker and the non-learning basis of this is bedrock for the ethnomethodological critique of contemporary social science. If all behavior is in fact organized in accordance with linguistic processes (or on a linguistic model) then all behavior is subject to the same problems of analysis. The arguments of Garfinkel and Cicourel do provide substantial support for this view.

There is no necessary implication that Chomsky's approach to language involves interpretive as opposed to mechanistic assumptions but the mysteriousness and depth of the remarkable system of generative rules seems to place the issue far beyond any present day theory that is buttressed intimately by data. Chomsky himself accepts a "mentalist" position which asserts that human linguistic behavior is not determined by external stimuli or internal physiological states. He seems for the most part to see a highly specific language "faculty" the operations of which are specified by its own working principles. If it is undetermined by external stimuli or by physiological states, there is great difficulty in understanding what basis there is for analysis (Lyons, 1970: 119-131). Chomsky's point is nevertheless compelling. He correctly makes explicit that some sort of generative faculty is necessary to account for very commonly observable language behavior. If it is difficult to see how such a faculty can be analysed then so much the worse for the would-be analysts. A pause is in order as some basic standpoints stand in need of reconsideration.

X. IS ANY ANALYSIS IN FORMAL FORMATS POSSIBLE?

Some general orientations toward further analysis seem based on the general proposition that whatever is involved in language (or more generally in behavior) there must be some basic structures involved that counts for its operating characteristics. In effect this position holds that at some point the "circuitry" will be examinable and this will settle all issues. Aside from any reductionism problems in this or any issue of "systemic relation versus component function of neural apparatus," There is a more basic issue of whether operation can in fact be stated from knowledge of circuitry. In their survey of the implications of cybernetics, Crosson and Sayre (1967), include an essay by J. L. Massey (1967) that points out an interesting outcome from the mathematical theory of digital computing machines. It has been determined that there is little control that can be effected over computing machines. For example it has been shown that it is impossible to formulate a test which, when applied to an arbitrary computer program with its input data, can determine even so little as whether or not the machine will every stop computing (Massey, 1967:64). The situation is yet more disturbing when computers are equipped with effector and sensor organs and can generate their own decisions about inputs. Though circuitry, programming, and data are known, the operation of the computer cannot be entirely predicted. Though something like a "Hal " in the motion picture "2001" is not implied by this, the point is that the examination of circuitry, even if at some point possible, cannot yield a finite formula for accounting

for the operation of even a complex manufactured mechanism much less that of language "faculty."

In a series of essays spanning some 15 years MacKay (1969) attempts to join the engineering and information sciences with the behavioral and social ones by marshalling concepts from the information sciences to clarify and organize ideas about such things as language, meaning, and choice. MacKay's essays are fascinating but come on their own to conclusions similar to Chomsky's by denying the possibility of a completely mechanistic theory of language (MacKay, 1969: Chapters 6 and 7). Interestingly enough for the position of the ethnomethodologists, the strongest basis for this conclusion seems to be his feeling that no mechanistic theory can match the flexibility of natural language in its adaptability across wide variations in identities of communicators and settings within which it is used (74-77).

XI. THE ACTUAL ACTIVITY OF SCIENCE PRECLUDES EXTENSIVE FORMALIZATION IN THEORY DEVELOPMENT.

The preceding paragraphs (1) accept Chomsky's view of language, (2) accept the position of ethnomethodologists that this view is a valid basis for understanding behavior in general, and (3) discuss some orientations that support the point that no mechanistic (formal deductive) model can account for behavior given the Chomsky-ethnomethodological assumptions. These points do not suggest that no account is possible only that a formal dedutive one is not. (Nor, to reiterate an early point, does it suggest that formal deductive thought modes are nowhere worthwhile. Such thought modes were seen as indispensible as rules of evidence in individual research enterprises.) The point of this paper is that the "casual abstraction" method is the best that we can do and constitutes the limit of the formalization strategy rather than its starting point. A small increment in formalization is possible with great effort but will never eventuate in axiomatic theory. It may well, however, reduce the ambiguity in earlier bodies of knowledge and this incremental factor, rather than aims of full formal statement, is probably the best way to understand the value of formalization.

For any given body of knowledge some degree of formal-style presentation is probably possible easily. Increments in formalization (i.e., increasingly explicit definitions, procedures, logic) will initially be easy also but if the world is like Garfinkel pictures it, the formal body will not only get very large and unmanageable but will also yield patterns in which many of same types of things will have different antecedents, and similar sets of antecedents will have different results. This is what systems theorists refer to as "equifinality" and multifinality" (Buckley, 1967:60). The attempt to find better and better "predictor" variables and "criterion" variables will be the response of the formal strategists. But if Garfinkel is correct, this will be analogous to the infinite regress that his students almost resorted to. Under identical conditions the interpretive process may not yield identical results. It can yield identical results even given widely varying conditions.

The issue is the process not the conditions and, as we have seen, there is no grounds to believe that this process must have a finite formula.

To follow through on our linguistic assumptions, it can be noted that no non-trivial situation has a fixed verbal formula. In the same kinds situations different sequences of utterances will have the same effect and the same sequence of utterances will have different effects. Most importantly, if it is a theory of action that we are looking for then our question is one of how actors (utterers) string together such sequences in active interchange in these situations. This question in effect is an appeal to some generative faculty or interpretive process.

It might well be asked whether, if this is so, there is not in fact a number of different situations involved rather than the same one. If a husband and wife are discussing whether to have a baby, is the situation the same when they are shouting at each other as it would be if they were discussing it in a warm and affectionate manner? Is any "baby-discussion" situation the same situation or must it be defined in terms of its tone? If the latter, why not use finer conversational distinctions in the definition? Why not linguistic ones? Such distinctions clearly relate to factors which have an influence on the outcome. But are they definitions of the phenomena?

The ethnomethodologists lean in the latter direction in their exploitation of the concept of "indexical expression " (Garfinkel and Sacks, 1970). Any utterance is "indexical" in their view in that it is rooted in the actual and specific configuration of factors that are the context for its use. In effect situations cannot be compared in any but the most wholistic manner. Any expressions occurring under different actual and specific configurations are different expressions even if they are identical verbal formats, (i.e., same sequence of utterances). Traditional sociology takes the opposite tack: a "baby-discussion" situation is a "baby-discussion" situation is a "baby-discussion" situation. The decisions which constitute the procedures for identifying which situations are "baby-discussion" ones are obviously subject to massive "ad hocing". Given this Wilson (1970) explores the dependence of deductive theory on an assumption of "literal description" (i.e., on the assumption that a baby-discussion situation can be literally and unambiguously identified) and concludes that since literal description is not possible deductive theory is futile.

The point is not at all that "ad hocing" is bad. The point is that it is inevitable in principle and would have to be included as a set of instrumentation premises in a fully explicit deductive theory. This would destroy the deductive relatedness of the theory because "ad hocing" is interpretive. This position does have important implications for the future of the social and behavioral sciences though it does not constitute a rejection of science itself. Douglas (1970:3-44) argues that commitment to such things as rigor and objectivity are not foresworn in the ethnomethodological position. The position does imply a rather strong determination to consider phenomena

in their full integrity. This means to consider phenomena in such complexity, completeness, and detail that the "indexicality" of communicative acts is explored. On the theoretical level the ethnomethodologists strongly assert that the natural science model of determinate or even probabilistic theory is impossible: "no doubt our objective knowledge will always remain partially grounded in the unexplicated situations of everyday use, but this is only to recognize that the scientific existence shares the ultimate absurdity of everyday life" (Douglas, 1970:44).

XII. TOWARD RECONSTRUCTION.

In the light of the points made in this paper, formal deductive strategies of theory development can be seen as quite arbitrary ones indeed. In fact, the claim that they have some special advantage in this direction either in terms of clarity or economy and efficiency in presentation is fatuous. They are convenient formats for recording findings in small to medium sized research enterprises in the social sciences but as a program for organizing a comprehensive fabric of knowledge they are useless in those sciences. As a group these strategies are essentially just another form of scientific rhetoric. This wording is harsh but it is difficult to consider seriously that formal strategies are somespecial, excpetional ones when in fact we are without any finished products to evaluate. Most attempts at formalizations are more respected for their imagery and methaphorical qualities than for their systematic deductive relatedness, firmness of conceptualization, and clarity of principles. As formal and deductive as we get in the social sciences, the equations and definitions, etc. are still almost always adjuncts to an ordinary prose language discourse. This is not to say that these formalizations are bad. They are often quite forceful and helpful. But the field of effect here is persuasiveness and not logical implication.

It is the position of this paper that formal deductive strategies constitute a form of ordinary language rhetoric as opposed to an alternative to it. The decision to employ formal deductive strategies is one made within the scientists interpretive processes but should not be viewed as a mechanism for escaping it. The point is that the scientist is using interpretive rather than formal processes for understanding the social universe. This being the case we as social scientists may be well advised to more directly employ interpretive processes in their full bloom. In a word: forget deductive theory and write ever more discerning explanations of social life in ordinary prose. Regard ordinary prose not as a stunted primitive beginning but as a relatively advanced format which supports understanding of the complexity which in fact exists. This interpretive form of understanding is uncomfortable because it gives us no sense of basic nature of explanation. Somehow a formal logical format gives us a bedrock feeling of the basic nature of explanation but it should be clear that this feeling is a cultural artifact and not a matter of reality. Interpretive understanding in principle is as good as any other; it is better if it can be more comprehensive.

Smart (1968:115) considers an instance of interpretive as opposed to formal explanation. Just knowledge in general about students furnishes an excellent interpretive basis for knowing about their beer drinking behavior. Observation would furnish even more knowledge about this. But it would probably be futile to seek to develop an axiomatic theory of student beer drinking behavior.

XIII. SOME RECOMMENDATIONS AND GOOD EXAMPLES.

As an initial implication of the counsel to "employ interpretive processes in their full bloom," it is important that we consider theories that see non-logical and interpretive operations as central ones.

In this direction seem to be a number of approaches which seem to assert a relatively clear progression from some primative mental characteristics to outward communicative behavioral manifestations. Although these approaches bear an outward similarity to the information theorists in their initial terminology, they move in a substantially different direction. Beginning with an initial distinction between "analogic" and "digital" information they depict the former in basically psychiatric images of mental process ("primary process") and the latter in familiar information theory terms. Communication then becomes a multiple dialectical process where analogical and digital components are constantly qualifying each other even as communicators interact in what they assume to be a simple and well-reasoned exchange of utterances. The general position of this view is summarized by Watzlawick, Beavin, and Jackson (1967)and its specific relationship with sociolinguistic and ethnomethodological positions is presented by Habermas(1970). The impact of these approaches is that they integrate a fundamentally nonlogical, affective, emotional view of man into an account of communicative interaction. They provide a speculation as to the nature of the content of the generative faculty and as to the placement of linguistic and behavioral activity in a context of arbitrary but natively perceived and informal (non-deductively entailed) implications. These sorts of analyses have been employed most directly as a psychiatric paradigm in the diagnosis and treatment of mental illness and as a theory of the etiology of schizophrenia.

These psychiatric-communication approaches are impressive because they account for a range of linguistic and behavioral phenomena that has not previously been integrated in a single body of theory. As such they furnish a sense of what is minimally necessary for an adequate account of phenomena that are either language-like or language dependent. They make clear that primitive mental processes are a component of everyday phenomena and that the depiction of such processes are interpretive and informal rather than formal. Most interesting though is the attempt to handle and depict these informal processes which furnishes a valuable clue to the structure and composition of informal theories. The approach of Watzlawick, Beavin and Jackson (1967) skillfully interweave complex mathematical and logical discussions with a complex interpretive discussion on language, communication, interaction, and family relationships. The arrangement is the opposite of that which is

usually desired. Instead of the formal material being assumed to be the heart of the matter and the prose material being tolerated as deformed and inexplicit what is in fact happening is that the prose is the heart of the matter and the mathematical and logical discussions are illustrations, almost pictures. They are not diagrams of the argument but are subunits in it.

A similar structure of theory seems to be characteristic of the recent trend toward systems theory. Buckley (1967) presents an extended essay which is not represented by a formal chain of logic but which is instead an extended persuasive discussion in ordinary prose which assembles and sorts mathematical and logical schemes into places in the interpretive scheme. In a second book (Buckley, 1968) more exhaustively assembles such formal schemes in anthology form.

A second basic strategy is substantive analysis. The archetype for this strategy is Dahrendorf's analysis of Marx (1959). This type of analysis may be regarded as an interpretive process applied to an earlier one. This is to say that it uses interpretive processes of discernment to clarify, disambiguate, sort and reinterpret a highly qualitative body of interpretive prose. It represents the incremental process discussed earlier. A somewhat similar approach may exist in Blalock's (1969) use of statistical methods to diagram verbal arguments. Blalock's immediate jump to formal methods but not on an axiomatic basis illustrates the essential equivalence of formal and interpretive methods in this incremental enterprise.

Still another strategy is that being undertaken by Hill (1971) which is an omnivorous attempt to organize insights which themselves might be regarded as interpretive ones. Hill's strategy seems to be one of grouping all prose presentations that have a bearing on the explanation of family phenomena into categories that reflect the conditions of their origin. It is obviously his intention to regroup them into more meaningful assemblages. The regrouping process is obviously quite difficult and is, just as obviously, not a finite one. This sort of enterprise bears some superficial resemblance to the propositional inventory idea discussed early in the paper. It is however, enormously different in that, while still a collection, it is a collection of complex interpretations rather than one of simple ideas of relationships between small numbers of variables. If Hill's strategy were to be based on formal deductive methods then the regrouping process would have to await full or nearly full formalization of all insights to be combined. Even then the insights would then have to be logically joined somehow. Either some would have to be subsumed under others or all would have to be implied under a new and vast general theory. This program would be ponderous beyond description. Whatever strategy is in fact employed, it will be accomplished with significantly less explicitness and with considerably more informality than a deductive format will allow. Paradoxically these are probably the only conditions in which the attempt would be successful. Hill and his group would do well to keep copious diaries so that these conditions can be explored.

Perhaps some looser formats might liberate some deeper and more powerful schemes. All strategies that have been discussed (in fact all of science) has been dependent upon a written-word-being-read format. But if open texture language is to be exploited as a source of interpretive insight then other formats may be more effective. A spoken-word-being-heard format or a dialogue might be more effective because these formats are more resourceful as fields for rhetorical effects. Bruyn (1966:125-159) points out that much of traditional conceptualization in the social sciences rests on rhetorical forms. Verbal discourse forms may liberate rhetorical structures that illuminate and more accurately depict phenomena than those yet exploited. More radically speaking, discourse forms may in fact be the supporting medium for whatever explanatory information there is to be generated. Consciousness raising among Women's Liberation units seems at times to be not only a format within which their doctrine comes to awareness but one in which it is documented. A written description of such a process is less impressive for its discursive content than for an idea of what feelings must have gone on there (see Pogrebin, 1973). It might be a mistake to assume that such data, being unreduceable to written formats, must remain entirely unavailable to persons engaged in scientific activity.

This shades into McLuhanesque issues in that visual presentations might be more resourceful. Any why stop there? Electronic presentations, multimedia presentations, etc. In effect the media is the message. If we are seeking to interpret reality there is no reason to insist that the schemes or models for doing so be printed linguistic ones that are confined to formal reconstructed logics. Any type of model might be serviceable so long as a correspondence between it and research operations are established. This proposal is not all that radical; what else is an analog computer.

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